VII.—*Description of an improved Anemometer for registering the Direction of the Wind, and the space which it traverses in given intervals of Time.* By the Rev. T. R. ROBINSON, D.D., Member of the Royal Irish Academy, and of other Scientific Societies.

Read June 10, 1850.

AMONG the various branches of meteorology, none has been less successfully cultivated than anemometry. As a necessary consequence, we are almost totally ignorant of the causes which originate and the laws which govern the currents of the atmosphere, notwithstanding their interesting character as objects of physical research, and their importance as cosmical agents. This, however, is not to be attributed to neglect; we find HOOKE and DERHAM pursuing the inquiry almost at the first dawn of physical science; and a variety of subsequent inventions connected with it shew that its importance was never forgotten. But a wrong path of observation was followed: the data which anemology requires are the direction and velocity of the wind at a given time; those which (with few exceptions) were sought, are its direction and pressure. Of the many ingenious machines which have been contrived for this purpose, those which are not mere anemoscopes may be reduced to three classes. In the first, originally devised by HOOKE, the wind acted on a set of vertical windmill-vanes, which are kept facing it by a vane, or some equivalent contrivance, giving them motion round a vertical axis. They turn till the pressure on them equilibrates a graduated resistance of some kind, whose amount measures it. In the second, a square plane receives the impulse of the wind perpendicularly, and thus compresses a spiral spring which is connected with it. This, which was invented about a century ago by the celebrated BOUGUER, has been lately brought into general use by Mr. OSSLER, who has much improved it, and made

it self-registering.* In the third class, of which LIND'S is the type, the pressure of the wind is measured by the column of water, or some other fluid, which it is able to support in an inverted siphon.

All these are liable to the following objections. First, wind fluctuates, both in velocity and direction, to an extent of which I had no conception till I entered on these researches. Instead of being a uniform flow of air, it may be likened to an assemblage of filaments moving with very unequal speed, and contorted in every direction; being in fact analogous to a river in flood, but with its eddies and counter-currents considerably exaggerated. Now, assuming the common equation $V^2 = mP$, we find $\frac{2dV}{V} = \frac{dP}{P}$, or the *relative* variations of pressure are twice as great as those of velocity; a record of the latter will therefore, be far less irregular. But the evil goes further; for in the fluctuations both of pressure and direction, the inertia of the moving parts of the anemometer carries them far beyond the point of balance, and makes the measure of pressure inaccurate, partly by exaggerating the amount of its changes, partly by the surface which receives the wind's impulse being at times wrongly placed with respect to its direction. The magnitude of this cause of error may be appreciated from these two facts, that I have seen LIND (the only pressure-gauge which I possess) range in a few seconds from 0 to 2·6 inches; and that in some winds a free vane will oscillate through arcs even of 120°.

2. The velocity can only be deduced from the pressure by experiment: if the relation between them be constant, this necessity is of little importance; but the fact is the reverse. In the case of the windmill-vanes, we have no information; and it is evident that the law which connects these variables must be very complex, in consequence of the wind which glances off the anterior surface modifying the *minus* pressure. In BOUGUER'S instrument it is commonly assumed that the pressure is the weight of the column whose height is that due to the velocity: there is, however, experimental ground for believing that it is nearly twice as great.† The excess is caused by the *minus* pressure,

* An anemometer of this kind, acting against a series of weights instead of a spring, was long used by the late Mr. KIRWAN, and is described in vol. xi. of the Academy's Transactions.

† See D'AUBUISSON, Hydraulique, p. 295. From DE BUAT'S investigations it is not unlikely that the velocities deduced from the records of OSSLER'S gauge are about one-third too great.

which, I may add, seems to follow a different law of velocity from the *plus* one. In LIND a similar uncertainty is produced by the negative action of the wind on the remote aperture of the gauge.

3. It has been well observed by FORBES, in his Second Report of the British Association on Meteorology, that little progress can be made in anemometry, except by the employment of self-registering instruments. If these record pressure, we cannot thence readily deduce the *mean* velocity, even admitting the law $V^2 = mP$. Let V' and P' be their mean values; $V' + v$, $P' + \pi$, any others; n their number, then

$$nV'^2 + 2V' \times S(v) + S(v^2) = n\,mP' + mS(\pi),$$

or as $S(v)$ and $S(\pi) = 0$,

$$V'^2 = mP' - \frac{S(v^2)}{n},$$

in which the last term is often of very great magnitude; or if we take

$$V' + v = \sqrt{m} \times \sqrt{P},$$

we have

$$V' = \sqrt{m} \times \frac{S(\sqrt{P})}{n};$$

but I have found the trouble of computing the sums of the square roots, even for a few minutes, an insuperable objection.

These seem to me sufficient reasons for absolutely rejecting the pressure-gauge, and adopting instead of it one which gives directly the velocity, or rather its equivalent, the space traversed in a given time. Instruments fulfilling this object are by no means of recent date. One was described in 1749 by the Russian LOMONOSOFF; it consisted of a vertical wheel with float-boards like an undershot, half of which was screened, and which was kept in the plane of the wind's direction by a vane. This, by a train of wheel-work, indicated on a dial the revolutions of the wheel; there was no provision for recording these in connexion with time, but a very ingenious one for noting the quantities of wind which blow from each point of the compass. A much neater one was constructed in 1783, by the late Mr. EDGEWORTH, and used by him to measure the velocity of air currents, though designed for a different purpose.* It con-

* For measuring the ascent of a balloon; two years later it was used by our countryman CROSBIE in his perilous ascent, and was preserved by him when the rest of his apparatus was lost

sisted of four light windmill-vanes, delicately mounted, the arbor of which had an endless screw, that recorded its revolutions by means of the elegant arrangement now called the cotton-counter. This, the invention of which is attributed by WILLIS to the late Dr. WOLLASTON (who, I believe, learned it from Mr. EDGEWORTH), consists of two wheels of n and $n + 1$ teeth, driven by the same screw; a tooth of the first passes a fixed index for each revolution of the vanes, and an index borne by it passes a tooth of the second for every n revolutions.* In 1790, the hydrometric fly of WOLTMAN was proposed by its inventor as an anemometer; but Dr. WHEWELL is the first who appreciated in its full extent the importance of the space-measure, especially in its giving an integral instead of a differential result. His memoir, published in the sixth volume of the Cambridge Transactions, marks an era in the science, and, in my opinion, indicates the only path of its progress. The instrument described by him has been used by several observers, but most extensively by one whose energy and talents were well adapted to establish its character, Sir WILLIAM SNOW HARRIS. The results which he exhibited to the British Association in 1841, while they fully proved the value of the principle, shewed, at the same time, that the mechanical details were not sufficiently perfect to carry out the views of the inventor: in particular, the space traversed by the recording pencil is (at least in moderate winds) not as the velocity, but rather as its square. HARRIS proposed to investigate corrections for this, which, however, would be different in each anemometer, and probably variable even in the same one. This error arises from the small size of the vanes, which have, therefore, too little power compared to the friction; while that is greatly increased by the same cause, as, from their great angular velocity, a complicated train of wheel-work is required to bring down the speed of the recording point to a manageable amount. This report induced me to consider the subject carefully; and as it seemed possible to correct the defect in question, and some others which I had observed in a similar instrument used by Captain LARCOM, R. E., at

in the sea. The wheels have seventy-two and seventy-three teeth, and the revolution of the second wheel measures a mile.

* A very convenient portable anemometer is made by furnishing a set of my hemispherical vanes with such a counter. In that exhibited to the Academy, the radius of the circle described by their centres = 5·6 inches, and the diameter of the hemispheres = 3·1 inches. If the number of revolutions which it makes in a minute be divided by 10, the quotient is the velocity in miles per hour.

Mountjoy Barracks, I obtained permission from the governors of the Armagh Observatory (who had already directed me to erect an anemometer) to carry into effect my views. After some preliminary experiments, I constructed in 1843 the essential parts of the machine, a description of which I now submit to the Academy, and I added in subsequent years such improvements as were indicated by experience. It was complete in 1846, when I described it to the British Association at Southampton; so that I have had sufficient opportunity to ascertain its efficacy.

In contriving it, I was guided by the following principles:

1. The moving power should be so great in comparison of the friction, that the correction due to the latter may be inconsiderable. It should also be easily applied.

2. One means of effecting this is to have surfaces which receive the wind's impulse as far from their axis of motion as is consistent with strength. This satisfies the second condition, namely, that they shall be acted on by a large section of the current, and thus give an *average* result. When the vanes are as small as those used by WHEWELL, they may give measures far different from the general velocity, if met by those partial streams to which I have referred.

3. The movement of those surfaces should be as slow, relatively to that of the wind, as may be consistent with a sufficiency of moving power: this lessens the train required to bring down the speed of the recording point, and also diminishes the wear and tear of the whole machine.

4. It seems desirable that it should act without requiring any special provision for turning it in the direction of the wind.

5. The structure should be such, that all made after the same type will give identical results.

The third, fourth, and probably the fifth of these conditions, are against the vertical windmill as a measure. Its vanes never move slower than the wind, often three or four times as fast at their outer extremities.* With the best guiding ap-

* This must generate a considerable centrifugal motion in the air dragged round with the vanes, which will complicate the direct impulse of the wind. Its effect in large windmills is illustrated by a remarkable fact, observed in Holland by the late Mr. NIMMO, that in some of the best of them, the weathering at the extremity of the sail is *negative.* This can only act by preventing the escape of the air. An effect of this kind must be difficult to calculate.

paratus, its motions round the vertical axis will not exactly correspond with the oscillations of the wind; and very trifling variations in the angle of the vanes will make a great variation in their speed. The fourth condition excludes those horizontal windmills which act by a moveable screen. Of the remainder, in one class the vanes are made to turn during the revolution, so as to present a diminished surface to the wind while returning against it; these are objectionable, because the necessary machinery is liable to derangement, and involves much friction, which will vary during a long period of working, and change the space unit. There remain then those only in which the vanes are curved, so as to be unequally resisted on their opposite surfaces. Of these, the most elegant in principle and definite in action that I know, was suggested to me many years ago by Mr. EDGEWORTH. Its vanes are hollow hemispheres, whose diameters coincide with the arms that support them; the action on their concave surfaces exceeds that on the convex so much, that the machine is capable of being used as a motive power with considerable advantage; its simplicity of form is such that, without very great exactness of workmanship, similarity of action can be attained; and it combines great lightness with strength sufficient to resist very severe gales.*

The relation between the velocity of its vanes and that of the wind can be determined satisfactorily, in the actual state of hydrodynamics, only by experiment. In this instance, however, the problem is so modified by the antagonism of the returning vanes, that theory gives not merely the law which connects them, but a close approximation to their ratio, and the correction due to friction.

Let AH be an arm of the machine, bearing the hemispheres AIB, DKH, and revolving in the direction of the arrows, so that the velocity of their centres $= v$.†

* In the gale of December 15, 1848 (the anemometer diagrams of which are among the specimens exhibited to the Academy), the space recorded during the hour $2^h \cdot 3^h = 61{\cdot}5$ miles, but during $2\frac{1}{2}$ minutes it is $= 4{\cdot}27$, which gives $102{\cdot}5$ miles per hour for the velocity of that squall. Short as it was, it did much damage in the neighbourhood, but the instrument was unhurt. A still heavier gust is recorded in the diagram of the cyclone of March 29, 1850, where the velocity is nearly 130 miles per hour for 3 minutes.

† At least this point is assigned as the centre of effect by the common theory; it may, perhaps, be a little further out in the concave.

This rotation *in quiescent air*, will cause a resistance to the convex surface of each hemisphere $= a'v^2$; a' being a coefficient depending on its diameter. To this the wind, supposed to act in the direction WE, adds another resistance on the convex of AIB; but it also acts on the concave of DKH, with a force which tends to increase v; and as its coefficient a is considerably greater than a', v will increase. In consequence of this, the concave surface recedes from the wind, and the convex meets it more rapidly; the impelling force, therefore, diminishes, and the retarding forces increase. To the latter must also be added the centrifugal force expended in producing an outward current in the air that is dragged with the convex surfaces, and the effect of friction. Evidently, therefore, a speed will soon be attained, at which these forces balance each other. If $\theta =$ the angle WEH, V the wind's velocity, we have, by the theory of BORDA for the undershot wheel,

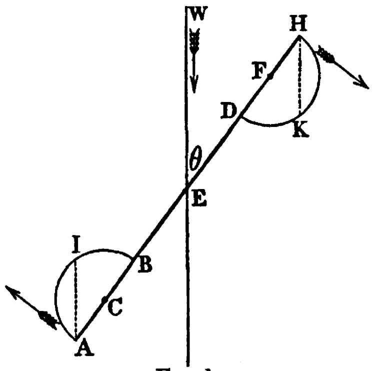

FIG. 1.

$$\text{Force on DKH} = aV^2 \sin^2 \theta - aVv \sin \theta.$$

$$\text{Force on AIB} = a'V^2 \sin^2 \theta + a'Vv \sin \theta.$$

The force due to the rotation alone $= 2a'v^2$, and the centrifugal force being as v^2 may be assumed $= 2b'v^2$. Let f also $=$ the moment of friction at C, then the actual impelling force

$$F = (a - a')V^2 \sin^2 \theta - (a + a')Vv \sin \theta - 2v^2 (a' + b') - f.$$

We must, however, take the *mean* value of this through the semicircle. It is

$$\int_0^\pi \frac{Fd\theta}{\pi} = \frac{a - a'}{2} V^2 - \frac{a + a'}{\pi} \times 2Vv - 2v^2 (a' + b') + f.^* \qquad (1)$$

* This reasoning supposes a and a' to retain the same value through the semicircle. Experiment shows that they vary; but as the change is greatest when their influence on the velocity is least, the error of this assumption cannot have much influence. The centrifugal force cannot act on the concave, as there is no tendency in the air which it holds to escape in the direction of the arm.

As their mean force vanishes when the condition of permanent rotation is attained, if we equate it to cypher, we deduce

$$\frac{V}{v}=\frac{2}{\pi}\left(\frac{a+a'}{a-a'}\right)\left[1+\sqrt{\left\{1+\frac{\pi^2(a-a')}{(a+a')^2}\left(a'+b'+\frac{f}{2v^2}\right)\right\}}\right] \qquad (2)$$

This shows that if we neglect the term introduced by friction, the ratio of the velocities V and v depends on the ratio of a and a' alone, being independent of their absolute magnitudes and also of v. It is, therefore, independent of the speed of the wind and the size of the machine.

Calling this ratio m, and making the instrument register $mv=V$, the true velocity of the wind $=V+u$, u being the correction due to friction, we have from (1)

$$\frac{a-a'}{2}(V+u)^2-\frac{2v}{\pi}(a+a')(V+u)-2v^2(a'+b')-f=0,$$

$$\frac{a-a'}{2}V^2-\frac{2v}{\pi}(a+a')V-2v^2(a'+b')=0;$$

whence

$$u^2+2u\times V\left\{1-\frac{2}{m\pi}\left(\frac{a+a'}{a-a'}\right)\right\}=\frac{2f}{a-a'}; \qquad (3)$$

the positive root of which may be tabulated for a series of values of V.

The constants of these equations must be given by experiment, and it is not easy to obtain them satisfactorily, especially the most important of them, a and a'. But for the unsteadiness of the wind,* both in force and direction, we might attach hemispheres to some weighing apparatus, with the concave and convex surfaces turned to the wind, and thus obtain absolute measures of them. This, however, could only be done by connecting the two with a pair of registers like those of OSSLER'S instrument, which would give the mean pressure for a considerable period; and such an apparatus is not at my command.

As, however, m depends on their ratio only, I found a method, which, though disturbed by the same cause, is tolerably successful. Two hemispheres, similar

* In illustration of this I may mention, that having placed two hemispheres on the arm, so that both concaves faced the wind (when, of course, they might be expected to remain in equilibrium), they oscillated with considerable force through arcs of 90°; the distance between their centres was 48·5 inches.

to those of the actual anemometer, are fixed on an arm in which, by means of a long slit, the axis of rotation can be shifted to any position; this axis causes a graduated circle to measure θ, the zero of which is determined by a vane above. The axis is shifted till the two pressures are equal, when, of course, a and a' are inversely as its distances from the two centres. In reducing this to practice, however, I found a difficulty which I had not anticipated. Since the forces on the hemispheres are as $V^2 \sin^2 \theta$, I concluded they would be at the maximum at 90°, and vanish at 0° or 180°; and began by observing them in the first of these positions. To my great surprise, I found that the equilibrium there is unstable, so that if the angle be changed the least either way, the concave predominates. This makes it hard to ascertain the true point of balance, as the direction of the wind is ever changing; but nevertheless I think I am warranted in concluding, with some confidence, from sixteen experiments made in four days with winds from a moderate breeze to a hard gale,

$$\frac{a}{a'} = 4{\cdot}011;$$

or, in round numbers, the action on the concave is four times that on the convex.

I was the more surprised at this predominance of the concave when the arm is inclined to the wind, because then the part HK of its convex acts against it.

From some other angles I obtained, though by fewer observations,

$\theta = 80°$, . .	$\frac{a}{a'} = 4{\cdot}128.$
75°, ...	4·378.
60°, ...	4·710.
45°, ...	4.783.
30°, ...	5·192.

Beyond this it is impossible to go, *as there the convex surface apparently ceases to act.* In fact, on removing the hemisphere DK, the other remains as in the wood-cut, making $\theta = 210°$ nearly, and oscillating as the direction of the wind changes. Whether this arises from the minus-pressure at the segment IA, or from the wind which passes at B eddying into the cóncave, I cannot decide;

but it is a striking illustration of the imperfect state of this branch of hydrodynamics.*

As to the coefficient b', its limits at least may be obtained by a process which I at first thought might give a and a',—the same which EDGEWORTH, HUTTON, BORDA, and VINCE, employed in their experiments on resistance. The resisting surface is placed at the extremity of a horizontal arm made to revolve round a vertical axis by a weight attached to a cord wound on the latter, and passing over a pulley. When the rotation becomes uniform,† the resistance must equal the accelerating force, and if this be constant, all the resistances must be equal, and, therefore, can be compared with the velocities. With this hope the apparatus described in the preceding section was placed in a tower of the Observatory, and alternately driven with its concave and convex surfaces foremost. Twenty revolutions were made before the time was noted; and then thirty were taken, giving $S = 381$ feet. The mean velocities varied

* From this it follows, that, if the friction do not prevent it, an anemometer of this kind should revolve even when its axis is in the direction of the wind. The small one already described does so, but this may be owing to the oscillation of direction.

† I fear the physicists just mentioned took the fulfilment of this condition for granted. This does not necessarily happen. Let F = the impelling power, K the moment of inertia of the apparatus, S the space described, T the time, V the velocity of the centre of the resistance.

$$\text{Accelerating force} = \frac{F - av^2}{K}.$$

$$v = \sqrt{\frac{F}{a}} \times \sqrt{\left(1 - e^{\frac{-2aS}{K}}\right)}.$$

$$T = \frac{K}{2\sqrt{(Fa)}} \times \log\left(\frac{\sqrt{F} + v\sqrt{a}}{\sqrt{F} - v\sqrt{a}}\right).$$

$$\text{Square of mean velocity or } \frac{S}{T} = \frac{F}{a} - \frac{K}{a} \times \frac{v}{T};$$

$$\text{mean square of velocity} = \frac{F}{a} - \frac{K}{a} \times \frac{v^2}{2S}.$$

From these expressions it is manifest, that v cannot be uniform till S is infinite. In my trials it continued to increase even till S was the largest I could command, 1143 feet. Hence also, the *mean square of velocity* (which ought to be used in computing the resistance) must differ from the square of the mean velocity; the latter, however, has always been used. As, moreover, no estimation has been made of the air's centrifugal force in the results which have hitherto been obtained in this way (and in fact it cannot be separated from the resistance), I am compelled to think they require revision, though they are at present received as standard facts.

from 2·23 to 8·50 feet. The values of F were determined by attaching to the centre of a hemisphere a fine thread perpendicular to its diameter, and passing over a delicate pulley (whose friction is known), to which weights were suspended, such that the driving weight just moved the apparatus when slightly jarred. These weights, divided by the mean squares of the velocities, give $a+b$ and $a'+b'$.* The result is

$$\frac{a+b}{a'+b'} = 2{\cdot}019,$$

and assuming $a = a' \times 4{\cdot}011$, we have

$$b' = a' \times 0{\cdot}9866 + b \times 0{\cdot}4953.$$

No means of determining the ratio of b to b' occurs to me; I could only satisfy myself that it is considerably less, by suspending a light body two feet outside the circle, and estimating the resultant of its deflection from the vertical in the direction of the radius. This made it evident that comparatively little air is thrown outwards by the concaves, the hollow, I suppose, carrying it round, and preventing its escape. We may, therefore, safely assume, that it is between the limits $b = b'$, $b = 0$, and much nearer the latter. These suppositions giving the limits $b' = a' \times 1{\cdot}9866$; $b' = a' \times 0{\cdot}9866$.

If now we substitute these values and that of $\frac{a}{a'}$ in (2), we obtain

$$\frac{V}{v} = 3{\cdot}306, \text{ if } b' = b; \quad \frac{V}{v} = 2{\cdot}999, \text{ if } b = 0.$$

It must, from what precedes, be much nearer the second; and if we also consider that the *mean* value of $\frac{a}{a'}$ through the semicircle is a little greater than that at 90°, we shall be justified in assuming the *theoretic* value of $m = 3{\cdot}000$. It is in very unexpected (by me) agreement with that given by experiment.

The most obvious mode of determining this constant—placing the instrument on a carriage, and comparing its record with the space actually traversed—

* It is assumed in this, that a and a' are the same as in a current of air, which, however, may not be the fact. Especially it is possible, that the air of the apartment may be dragged round with the cups, and thus offer less resistance.

I found to fail, partly from the difficulty of eliminating the action of the wind, but still more from the fact, that a carriage *drags with it a quantity of air*, so that for many feet from it the anemometer does not feel the full effect of the motion. At low speeds, and on days of calm, I have got results which agree with that given by other methods, but more frequently the discordance destroys all confidence in it. The aerial log proposed by Sir W. S. HARRIS in the report at Plymouth, could not be applied, on account of the lofty position of my instrument; but I tried one far more delicate, by exploding small charges of powder at it, while my assistant noted the time required by the little globes of smoke (which in dry weather are not dissipated for many seconds) to traverse 150 feet. But the irregularity of the wind's motion makes all such trials unsatisfactory, and I got the most discordant results, the reason of which was evident by watching the track of the smoke; it rose, descended, twisted in eddies, and even occasionally came back many feet against a strong breeze. But in addition it can only give the movement of that *one* part of the current which it occupies, while the anemometer shows those of all that pass it in the same time, which are essentially distinct. I may add, that the impossibility of obtaining accurate measures of velocity by such means, was long since pointed out by Mr. BRICE.*

The plan which succeeded consists in applying the whirling apparatus to carry the anemometer, as in the annexed figure. The anemometer has four hemispheres; it is similar to the actual one, and about a fourth of its dimensions: the distance AB is 45·6 inches, and as the diameter of the hemispheres is only 3 inches, we may assume the velocity of their centres to represent that of the wind. C is a counterpoise. I found that in this case the rotation produced no important outward current. The machine

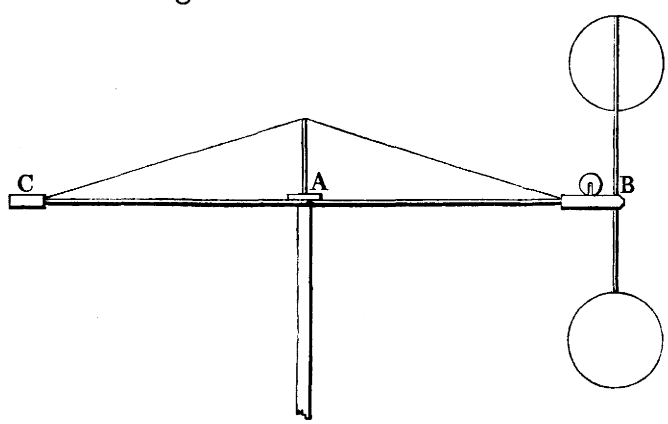

FIG. 2.

* Phil. Tra ns., 1766.

was permitted to make a few revolutions to come to its speed; and then the counter was put in action for a certain number of revolutions of the arm AC, generally 96. The time was also taken to give the mean velocity. I found

Driving Weight.	V in Feet.	$\frac{V}{v}=m.$	No. of Observations.
6 lbs.	7·09	3·562	6
15 „	11·53	3·133	13
21 „	13·75	3·004	9
27 „	15·66	3·004	7

I did not venture higher velocities, as the apparatus was not strong enough; but the above are sufficient to show that, after allowing for friction, the value of $m = 3{\cdot}000$.*

* Some facts observed during these experiments may be deserving a record.

1. The mouths of the hemispheres being covered with paper, so that planes were substituted for concaves, I found, with $V = 12{\cdot}80$ feet, $m = 5{\cdot}041$.

2. Cutting away the central paper, so as to leave merely a ring 0i·2 broad, which (from what has been observed with PITOT'S tube) I thought might increase the effect, proved very disadvantageous.

3. Making the cups segments of 220° was also hurtful, for, with

$$V = 13{\cdot}87, \quad m = 5{\cdot}220.$$

4. A single hemisphere with a flat counterpoise presenting its edge to the air, gives

$$V = 7{\cdot}30, \quad m = 3{\cdot}700.$$

5. With three arms, two carrying *entire spheres*, and one a hemisphere,

$$V = 10{\cdot}30, \quad m = 7{\cdot}900.$$

6. Five vertical windmill-vanes (the best number), of the same outer diameter, but heavier, and set at 45°, give, in 96 revolutions of the arm,

$$V = 7{\cdot}63. \quad \text{No. rev.} = 497{\cdot}5.$$
$$V = 12{\cdot}29. \quad \text{No. rev.} = 516{\cdot}0.$$

The four hemispheres at the same time,

$$V = 13{\cdot}61. \quad \text{No. rev.} = 141{\cdot}2.$$

The tips of the vanes here move about 3·4 times as fast as the wind.

That this ratio holds for the large anemometer, as well as the small, is experimentally shown by placing the latter beside the other, and counting the revolutions made by it during eighty-eight of that. Fourteen such trials give, with a mean velocity of wind = 15·6 feet, the ratio of the revolutions = 4·12, the inverse of their dimensions being 4·29. The difference is due to the large one being above the other, and therefore getting the wind more freely.

As this relation does not depend on the elasticity of the impelling fluid, it should hold when the instrument is acted on by a stream of water, with the advantage of being much less affected by friction. I tried this in a large mill-course near Armagh, placing the small instrument in the central part of the current, where the velocity was found by floats = 1·613 feet. I obtained,

With four hemispheres, . . . m = 2·972.
With two, ... = 3·208.
With three, equidistant, ... = 3·041.

The trial with two was made rather from curiosity than from any dependance on the result which might be obtained, as, when passing the line of centres, the impelling force is so slight, that any eddy will produce a disturbance of the motion. It would not change the mean much, but I think should be rejected. That of the other two is 3·006, still probably a trifle too large, as the four are preferable on the same grounds to the three.

From all this I think we are warranted in laying down this law, that in a horizontal windmill of this description, the centres of the hemispheres move with one-third of the wind's velocity, except so far as they are retarded by friction.

This principle once established, its application is easy. Plate IV., fig. 1, shows the external appearance of my anemometer, as it stands on the flat roof of the dwelling-house. Its frame consists of four uprights, 3^{i} by 2^{i}, and $15^{f}\ 4^{i}$ long; $6^{f}\ 5^{i}$ asunder below, $2^{f}\ 4^{i}$ above. They support the strong frame B, in which a diagonal carries the bearings of the axles C and D. The part H is sheathed and roofed with plank (the roof covered with painted canvass); and it forms a very convenient room for the self-registering apparatus. The copper funnel F is attached to each axle, to prevent the entrance of wet. The great height of this frame is necessary to clear the dome of the west equatorial, which rises

S. E. of it; but it has the double disadvantage of causing additional friction by the weight of the long axles, and making the whole less stable. To obviate this last defect, about 3 cwt. of pig ballast is disposed round the floor of H; notwithstanding which, the machine was blown down in March, 1845. After this it was further secured by three iron shrouds attached to the walls in the directions S. E., S. W., and N. W.; and it has since withstood still heavier gales. The axle C bears the mill G for space; the axle D the vane V for direction.

The dimensions which I chose for the first of these are, 12 inches for the diameter of the hemispheres, and 23 inches for the distance of their centres from that of the axle. The latter might, perhaps, have been increased with advantage; but I was afraid of weakening the arms too much. The hemispheres are made of sheet zinc, strengthened by a wire rim; each weighs 1·31 lbs., but might have been lighter if made of thin copper. The arms which carry them are iron, 1·5 inches broad, and 0·1 thick, but feathered off to a sharp edge at each side, and kept from bending downwards by stays of wire. The hemispheres are four; for I found, by trials with the small anemometer, that this number is better than either five or three. Six is inferior to any lower number, not excepting two; probably because some eddy from the concaves reaches the convex surfaces. The iron tubes T, 8 and 18·5 inches long, are secured to the diagonal of the top frame, and carry boxes of bronze, in which are bronze balls, on and between which the axles C and D turn. This arrangement is the result of many experiments. At first they turned above in common brass journals, and their hardened points rested below on surfaces of hard steel. As, however, C with its appendages weighs 20·69 lbs., and makes, on an average, 1500 revolutions per hour,* the bearing surfaces were soon abraded; the friction also was far too great, being equivalent to 104 grains acting at the centre of a hemisphere. I then refashioned the pivot very carefully, and set it in an agate cup; but, though this was kept full of oil, after a year's work, I found that a hole of some depth had been drilled in it. I substituted for it one of sapphire, but even this failed after two years; and the friction was not so much lessened as I expected,

* Of this 6·69 is due to a piece of iron tube composing C, which I have recently replaced by a shaft of deal; this has reduced the weight to 16·23 lbs. The average velocity of the wind is about 10 miles per hour.

being 73 grains. This was finally replaced by the present mounting in May, 1849. It is shown in Plate V., figs. 2 and 3, where C is the axle, 0·82 inches diameter, D the box of bronze (8 copper to 1 tin); B, five balls of the same, 1·12 diameter; I a disc of iron truly turned on the axle; H an aperture for introducing occasionally a few drops of oil, which I find necessary for the lateral action of the balls. They bear both the lateral pressure and the weight; and, therefore, require only a slight lateral support below, which is given by the arbor of the endless screw. This arrangement shews no trace of wear after more than a year's work, and the total friction is but 53 grains:* the coefficient of that part of it which belongs to the balls, I find to be $\frac{1}{325 \cdot 6}$ of the load. As in high winds there is added to this a lateral pressure, in respect to which the balls do not act quite so advantageously, we may take it $\frac{1}{300}$. From this value of the friction, the correction of V is easily computed;† but it is in some

* Of this I find that 20·36 belong to the mill, and 32·64 to the registering apparatus: with the new shaft the total friction will be 48·61.

† For this it is also necessary to know the constant $a - a'$. I approximated to this as follows: a spring-balance is attached to a cord wound on the axle C, which, as $v = o$, measures with *four* cups the force $V^2 (a - a')$. Its slide moves a pencil parallel to the axis of a cylinder covered with paper, and made to revolve by clock-work, on which it traces the curve of time and force. The small anemometer already described gives V by comparison with the time. This V is reduced to that of the large instrument by comparative trials at the time of experiment. It must, however, be remembered, that it is a *mean velocity*, and that, therefore, the value of $a - a'$ thus obtained, is too small if the fluctuations be considerable. As V is affected by friction, the first values of $a - a'$ are used to correct it, and thus a more exact result is given by a second computation. By six diagrams I find,

Time = $126^s \cdot 2$;	$V^2 (a - a') =$ 828·6 grs.;	$V = 4^m \cdot 99$	$a - a' =$ 33·28 grs.
98·8;	1760·6;	10·40	16·22
96·6;	1168·1;	6·27	29·71
184·2;	685·1;	2·85	51·57
101·2;	555·8;	5·04	21·04
98·0;	1698·7;	6·81	36·03

The second and fourth were marked as doubtful from excessive fluctuations; but as the mean of them and the fifth differs little from that of the other three which were considered satisfactory at the time of experiment, I retain them, and take $a - a' = 31 \cdot 997$, or 32 in round numbers. The equation (3) becomes, with the values previously given for $\frac{a}{a'}$ and m,

respects preferable to correct mechanically by applying to C an auxiliary force equivalent to the friction. Besides the saving of labour, it extends the action of the instrument; as, from the data in the preceding note, it cannot move with less than 1·29 mile per hour. The dynamic effect of such a force, while the wind traverses 100 miles, $= \frac{53 \text{ grs.} \times 5280 \times 100}{3 \times 7000} = 1332 \text{lbs} \times 1$ foot; it would, therefore, require a weight of 37 lbs. falling 36 feet. The locality does not permit this; and I, therefore, purpose to use a remontoir, wound up by a small mill similar to the anemometer itself. Perhaps an electro-magnetic machine might be simpler; the expenditure of zinc and acid would be trifling, and their consumption proportionate to the work done. The chief difficulty would be the inconstancy of the current.

The vane V is three feet long by one and a half extreme breadth; it also is made of sheet zinc. From a wish to give it as little momentum as possible, it was at first a light wooden frame covered with varnished calico, which the wind soon destroyed. This axle turns also on balls.

It remains to describe the self-registering apparatus; and first, that for the space.

My first intention was to adopt a form resembling the charts of wind-paths given by Dr. WHEWELL in his memoir, but in which the curves should be drawn by the wind itself. The arrangement I proposed was to make the

$$u = V \times 0{\cdot}64637 \left\{ \sqrt{\left(1 + \frac{f}{a - a'} \times \frac{4{\cdot}78705}{V^2}\right)} - 1 \right\}; \tag{4}$$

which, with the above values of f and $(a - a')$ is

$$u = V \times 0{\cdot}64637 \left\{ \sqrt{\left(1 + \frac{7{\cdot}92855}{V^2}\right)} - 1 \right\}. \tag{5}$$

From this the following table is computed:

V	u	V	u
$V = 1^m$	$u = 1^m{\cdot}285$	$V = 6^m$	$u = 0^m{\cdot}068$
2	0·469	7	0·050
3	0·240	8	0·039
4	0·144	9	0·031
5	0·095	10	0·025

It is evident that above 5^m per hour the correction is insensible.

plane which holds the paper move by means of two slides at right angles to each other. Its motion should be given by a rack, travelling proportionally to the space, but revolving so as to be always in the direction of the wind. A pencil placed over the centre of its revolution would describe on the paper a track perfectly similar to that of the wind. At each hour and quarter hour, a clock was to print by punches a series of marks, which would represent the time.* The mechanical arrangements were planned, and certainly this method would have the great advantage of showing at one view the three variables, and speaking most distinctly to the eye; but I gave it up, from a conviction that it is much less adapted to give periodical means than the method of co-ordinates.

Of these I prefer the polar to the rectangular, for the following reasons. In the first place, the direction, being an angle, is at once recorded; secondly, a movement of rotation can be given to the paper-holder with much less friction than a rectilinear one; thirdly, this movement may be continued through many circumferences without inconvenience, while the other is limited by the length of the rack, or other contrivance for producing it. In the rectilinear direction-register there is also the inconvenience that, if the wind veer several times in the same direction round the horizon, a new series of graduations must commence. I may add, that there is, perhaps, a want of graphic propriety in representing angular veering by a right line, but none in measuring miles by a graduated arc. Fourthly, one form of printed paper serves for both. The only objection to the polar form, of which I am aware, is, that the scale is less near the centre than at the circumference; this, however, may be obviated in any case, when it is desired, by winding up the apparatus at shorter intervals, so as to keep the pencil near the latter.

First, then, as to the space: the dimensions which I have adopted for the windmill are such that, in 440 revolutions, the hemispheres travel one mile,

* August 16th. This, I find, has been applied by Mr. OSSLER, who showed me at the late meeting of the British Association, some beautiful wind-curves, where the time is thus expressed. He checks the excursions in direction by using a windmill, and with great success. The x of his curves is the space, the y the direction. The time is shown by dots, single and multiple, struck in pairs at each side of the paper, and its record is very complete.

and the wind three. If degrees on the paper be miles of wind, the number of the former must be 440 × 120 for one of the paper-holder. A train which effects this very simply was arranged for me, by one whose recent loss I lament, not merely from personal regard, but from regret that science is deprived of aid so powerful as that of his high mechanical talents,—the late Mr. RICHARD SHARP. It is shown in Plate VI., fig. 4, where A is an arbor held loosely in the lower extremity of the axle, and carried round with it by the screw *c*. An endless screw on this drives the wheel B, of 88 teeth; a second endless screw S drives C, of 100; its pinion D, of 16, drives E of 96. On this the brass plate P, 14 inches in diameter, is fastened by a steady-pin and the nut H, which also assists in holding down the paper. The speed of the train is therefore $= 88 \times 100 \times 6 = 440 \times 120$.

The arrangement for direction is shown in Plate VI., fig. 5. The arbor F (which is also loose in the hollow of the vane axle) bears the wheel G of 96, which drives K of 96. On this the paper-holder P′ is secured by H′; its angular movement is therefore equal to that of the vane, while the paper can be more easily removed than if it were immediately carried by the vane-axle.

That axle is connected with the arbor F, not by any rigid attachment, but by the spiral spring L. This is necessary, not merely to prevent the destruction of the machinery in violent oscillations of the vanes, but still more to lessen their extent on the register-paper. Though Dr. WHEWELL had pointed out the magnitude of these oscillations, and the impossibility of preventing them, I was not at all prepared for what I found. It may be that these waverings of the wind are of greater amount at Armagh than elsewhere, owing to the exposed situation, and the undulating surface of the country; but, without some contrivance to check them, the direction-papers would be very unsightly objects. It must, however, be remembered, that they cannot be avoided entirely, nor is it desirable that they should be too much diminished; for I find that this is a distinctive character of some winds, independent of their velocity, and, therefore, implying some peculiarity in the origin or progress of the current. In particular I have remarked, that when excessive, it is connected with a roaring sound, that gives an exaggerated impression of their force. This was strikingly exemplified in the destructive tempest of February last, whose highest velocity did not exceed 40 miles per hour. On another occasion, when the

velocity was of nearly the same amount, the sudden diminution of roar led me to suppose the gale was abating; but, on going to the instrument, I found the velocity had increased to 52, while the range of direction was only half its previous extent.

The contrivances which I have applied as checks on the direction-fluctuations seem to work well. In the first place, such as are completed in a second or two are chiefly expended in bending the spring L, being past before its tension can overcome the inertia of the paper-holder and its machinery. Secondly, the wheel G drives a regulator attached to the arbor of the pinion I, but not shown in the drawing. This consists of four vanes, shown in plan, Fig. 3, made of light deal frames covered with paper. Each is 37 inches high and 15 broad. As the whole is very light and turns on an agate, it yields to the slightest impulse of the vane, *if time be given*, but presents a very great resistance to rapid motion. Its speed is $\frac{96}{10}$ times that of the vane,* and this, combined with the action of the spring, will often reduce the oscillations to one-third of their absolute magnitude. As at first applied, the regulator was much smaller and immersed in water; but I was obliged to abandon that plan, in consequence of its action being interrupted during frost.

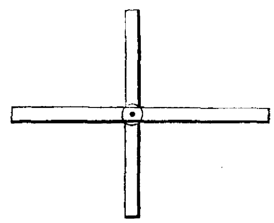

Fig. 3.

Lastly, Plate V., fig. 6, shows the method of connecting these two registers with that of time. N is a cast iron plate which bears the whole machinery, 40 inches by 14. P and P′ are the paper-holders; each has three spring-clips at its circumference, to hold the paper, which is further secured by the screw H passing through a hole punched in its centre; this screw serves also to centre it, being of the same size as one of its circles. One of these clips bears a fiducial line, with which the zero of graduation is made to coincide when a new paper is applied. M is a common clock movement, the weight and pendulum of which pass through openings in N. Its barrel carries a second wheel, which moves, by a rack, the bar pp' through six inches in twelve hours. This

* I have since added an intermediate wheel and pinion, which makes the speed $\frac{96}{10} \times \frac{30}{10} = 28{\cdot}8$, which is a considerable improvement. 24 might be immediately obtained, and would be, perhaps, the best.

bar slides in a dovetail on the front plate, and carries adjustable tubes at its extremities, in which pencil-holders are placed, and made to act by weights placed in their cups. Since, however, in the direction-register, the pencil, as at first arranged, and shown in the figures, travels from the circumference, I have found that in damp weather it occasionally has pulled the paper from the clips and torn it. I have, therefore, lately carried it by an additional piece, one end running in a guide at O, the other provided with a stud which fits in p': this complicates it a little, but remedies the inconvenience, and makes the time-reading the same in both registers.

The paper used is printed in red, from a plate engraved with a graduation of degrees and half degrees. Within this are a series of concentric circles, which represent portions of time. Those which correspond to hours are stronger than the rest, and half an inch apart; the intermediates show decimals of the hour. The mode of using it is this: the pencil p' being removed, the date is written on P near its pencil; the clock is then wound up, and p draws a line from the circumference to the centre. The paper on P′ is then removed or shifted, and if another be placed, it is similarly dated, with the addition of the degree, which is set at the fiducial line; and the pencil p' is replaced. Then, during the ensuing twelve hours, the action of the clock carries the pencils from the centre to the circumference. If there were no wind they would merely draw radial lines; but in general p traces a spiral, and p' shades an irregular sector. The clock should be adjusted so that the twelve hour circles should be exactly traversed. In general, a space-paper may contain four or six spirals, dating each winding line; and a direction one, two, or three sectors, shifting the zero point for each. This zero in my practice represents a wind from the south, and the graduation goes round from west to north. The papers are finally fixed with a weak solution of mastic in common whiskey, and preserved for reference.

In reducing these diagrams to a form available for computation, I have found no system preferable to the method pointed out by Dr. WHEWELL in his memoir. In the first instance, the centres of the papers are restored; in the space-papers, drawing radii through the intersections of the spirals with the hour-circles, the graduation gives the hourly spaces, which, if necessary, are corrected for friction: these are tabulated. In a second column is entered the

direction at each hour. This is found by bisecting the arc of the hour-circle, which is shaded by the pencil.* The mean direction during each hour will, in general, not differ from the mean of those at its beginning and end; but if the eye perceives that this is not the case, those for the decimals of the hour may be taken. From this are computed two rectangular co-ordinates, which are given in the third and fourth columns; w the motion of the wind from the west, s that from the south. These are obtained by multiplying the hourly spaces into the sine and cosine of the mean direction. I have found it easiest to do this by a large sliding rule, having arranged a table of sines and cosines for each decimal of the degree. They need only be to three places of decimals, but should have a quadruple argument; its first column from 0° to 90°, its second from 180° to 270° (these on the left): its third from 360° to 270°; its fourth from 180° to 90° (these on the right): and over each column the appropriate signs. Details of this kind may seem trifling, but the waste of labour which they avoid is of great consequence when so great a mass of work has to be performed, as even one year of such a registry involves.

From these co-ordinates any final results may be obtained, as hourly, daily, monthly, or yearly means. Let W be such a mean of w, S of s, attending to the signs; then D, the mean direction for that time, and Σ, the mean space, are given by the equations

$$\tan D = \frac{W}{S}; \quad \Sigma = \frac{S}{\cos D} = \frac{W}{\sin D};$$

remembering that $\sin D$ has the same sign as W, and $\cos D$ as S, from which the quadrant of D is known.

As an example I annex the reductions of the twelve hours during which the centre of the cyclone already referred to passed the Observatory, as one which will illustrate the process in an extreme case.

* This is most rapidly performed by a plan explained in the figure. Let BC be the arc of the hour circle H; lay an edge of the ruler RT through C, and the centre I, so that its extreme point is on the hour circle. Then lay the parallel-ruler PL through that point and B; remove TR, and move the half of PL till it passes through I; the point G is in the line bisecting BC.

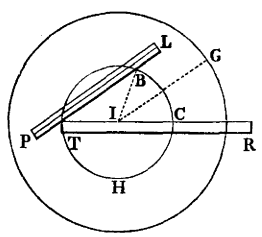

FIG. 4.

Date.	Space.	Direction.	W.	S.
March 29, 10 P. M.		$303^{\circ}\cdot8$		
11	$33^{m}\cdot5$	313·8	$-26^{m}\cdot1$	$+21^{m}\cdot0$
12	32·0	320·5	− 21·8	+ 23·4
1 A. M.	31·1	307·3	− 22·4	+ 21·5
2	29·4	314	− 22·3	+ 19·1
3	30·3	294·7	− 25·0	+ 17·1
4	31·5	$\overline{77}\cdot2$*	− 19·9	− 24·4
5	30·5	78·8	+ 29·9	+ 6·3
6	31·1	66·7	+ 29·7	+ 9·3
7	31·1	69·1	+ 28·8	+ 11·7
8	32·9	$\overline{88}\cdot2$	+ 24·5	− 22·0
9	33·6	88·2	+ 33·4	+ 1·0
10	37·5	99·9	+ 37·5	− 0·3
Sum, ...	385·5		+ 46·3	+ 83·7

The means for the two irregular hours are taken from the reading for each tenth. We have $\tan D = \frac{46\cdot3}{83\cdot7}$, which, as both are positive, must be in first quadrant, therefore,

$$D = 28^{\circ}\cdot95, \text{ and } \Sigma = \frac{46\cdot3}{\sin 28^{\circ}\cdot57'} = 95\cdot65.$$

It appears, therefore, that during these twelve hours, the real movement of the air was only 95·6 miles, from a point 29° west of south.

For all purposes of physical investigation, this method of exhibiting the results is fully efficient; at the same time it is much to be desired, that some graphic method could be devised which would exhibit to the eye the relation

* At $3^{h}\cdot30^{m}$ the wind veered suddenly 217°·5, against the order of graduation, which is shown by the sign —. The mean direction for the hour = 219°·2. There also was at 7^{h} *exactly* another veer, in the same direction, of 210°·5. The mean direction for the hour = 132°·0.

of the space, direction, and time at one view. This might in some degree be performed by a delineation of the actual trajectory of the wind, either drawn by itself, or laid down from the co-ordinates w and s, on which the corresponding times are marked; but the analogy of curves described on a plane, and expressing the relation between two variables, naturally leads to the notion of a solid whose three dimensions would afford a triple representation. It would involve the construction of a model, or at least a contoured plan. For, in fact, if we conceive perpendiculars to be raised on one of my direction-papers, at each point of the shading, proportioned to the velocity at the corresponding instant, their totality would be limited by a relieved surface which would show by its undulations the state of the aerial movements, and might be contoured. Unfortunately, the changes of direction are so abrupt and large, that it is absolutely impossible to exhibit in this way the conditions of any short period; but it is probable that it may be different with the hourly or even annual mean of a considerable number of years; and I venture to recommend it, or some equivalent, as an object worth the attention of meteorological inquirers.

T. R. ROBINSON.

ARMAGH OBSERVATORY,
June 8, 1850.